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A N  
INAUGURAL DISSERTATION  
O N  
*A B S O R P T I O N.*

SUBMITTED TO THE EXAMINATION  
OF THE  
REV. JOHN EWING, S. T. P. PROVOST.  
*The Trustees and Medical Faculty*  
OF THE  
UNIVERSITY OF PENNSYLVANIA,  
FOR THE DEGREE OF DOCTOR OF MEDICINE.

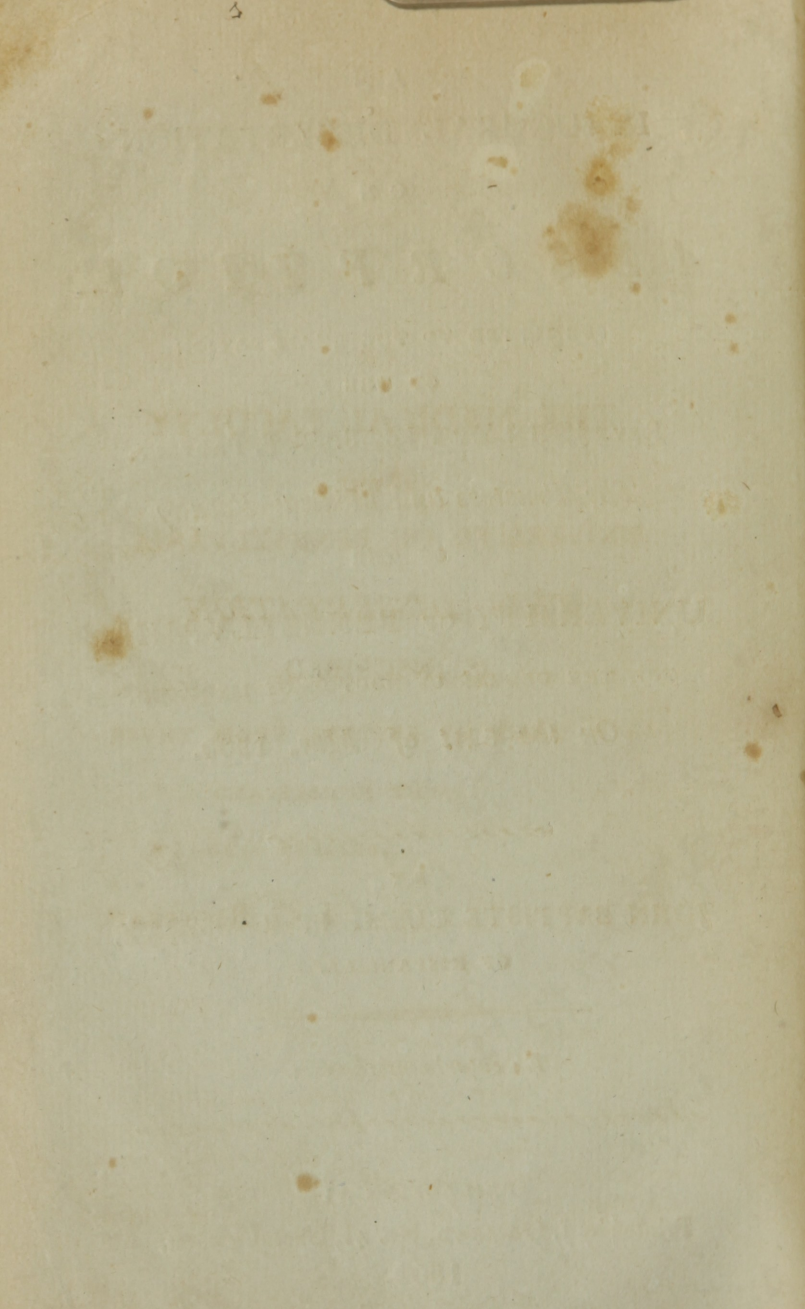
*On the 31st of May, 1800..*

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B Y  
JOHN BAPTISTE CLEMENT ROUSSEAU,  
OF HISPANIOLA.

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*Ex experimentis verum.*  
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TO  
THE MEDICAL FACULTY  
OF THE  
UNIVERSITY OF PENNSYLVANIA,  
*THIS DISSERTATION*  
IS INSCRIBED,  
AS A MARK OF ESTEEM, FROM THEIR  
MOST HUMBLE AND  
OBEDIENT SERVANT,  
J. C. ROUSSEAU.

C







## INTRODUCTION.

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NO subject, perhaps, is more worthy of the investigation of a Physician than ABSORPTION. A knowledge of the manner in which it is performed, would lead him to the source of the remote causes of a vast number of diseases; point out the mode of prevention by the removal or destruction of such causes, and direct him in the application of remedies according to the seat of the disease.

Too much has indeed been attributed to the absorption of remedies into the mass of the blood. Many of them produce simply a local affection, without being at all carried into the general circulation. This local affection is sometimes extended to other parts, but only by sympathy or a connection of parts, through the medium of the nerves; and I do not hesitate to say, that in more than one half of the cases, the effect of remedies is simply mechanical; that it operates by creating a new action in the system, which action, if stronger than the first, either removes, suspends, or destroys it\*.

\* This action may either be direct or indirect. It is direct when the remedy acts immediately upon the body; indirect when it acts by the interposition of the mind, which, by its intimate connection with the body, transmits to it an action which no remedy could have produced upon the animal fibres. Often, the action

Owing to this deference for absorption, a multitude of lotions, liniments, ointments and medicinal baths have been introduced into medical practice, all of which are at present much disregarded, and will sooner or later be, like unguents and plaisters, entirely set aside.

Diveſted of all prejudices concerning the preſent doctrine of abſorption, and deſiring to aſcertain in what manner morbid exhalations are introduced into the body, I have thought that there was no other way to find out the truth but by experiments: that to repeat and vary them, was abſolutely neceſſary to remove all doubts upon the exactneſs of their reſult, and that nothing could better enable the reader to form a ſound judgment, than a minute detail of my operations. I have truſted in nothing to authorities, and a conſtant and ſtrict obſervation has uniformly guided me.

Had nature been conſulted more frequently, not ſo many errors would have been propagated upon authorities. The phyſician ought never to be governed by prejudices: he ſhould take his judgment for a guide in his obſervations, and ſettle his doubts by experiments. Every thing in nature ſolicits his attention; by ſtudyng her in her operations, he will learn to imitate her; to aſſiſt her

produced upon the mind extends no farther, than favouring cuſtoms and habits which become beneficial, or checking thoſe which are detrimental to the conſtitution. Thus we can explain why a number of inſignificant remedies, ſuch as the carrying of a dry toad in a leather bag for preventing the return of the paroxyſms of the gout, the flint ſtone water for the cure of obeſity, &c. &c. have found ſo many advocates. When a ſevere diet or faſting is preſcribed during the uſe of the flint ſtone water there is no doubt that it muſt prove efficacious in retarding the progreſs of obeſity.



when her power is deficient, and to counteract her when she creates a wrong action; he will be enabled to distinguish the operations of remedies from those of nature; for, although she does seldom work in secret, she escapes by her constant activity from our slow inquiries; and from the hardships and difficulties which are met with in prosecuting her study, conjectures are substituted for facts, and thus the healing art is often rendered more pernicious than salutary.

I have, in the following pages, confined myself most-ly to the recital of my experiments, and of their result. I have said nothing but what may be relied on, omitting to relate a great number of experiments, which in my mind did not appear entirely free from error. But as, notwithstanding the greatest care that I have taken in performing and repeating those recited here, knowing very well how easy it is to be deceived in matter of experiments, some errors may have slipped in, I do declare that they are to be imputed solely to some concomitant causes which I was not able to perceive.

The pleasure of discovering new truths has engaged me for a long time, in the present inquiry, and the hopes of receiving some informations upon it, induced me to make it the subject of an inaugural dissertation. I am not unaware of the numberless objections which may be presented against it, but as I ground nothing but upon facts, I may reasonably expect that facts alone can militate against me.

It is not, I must own it, without the greatest diffidence, that I venture to write in a language, which but few years of practice have hardly rendered familiar to me. My principal aim has been to express my sentiments with simplicity and clearness, if I have attained to it, whatever may be deficient in elegance of style, will, I hope, be excused with that indulgence that any sensible reader would claim for himself in my situation.

J. C. R.



A N  
EXPERIMENTAL DISSERTATION  
O N A B S O R P T I O N.

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THE functions of the absorbent system are yet far from being ascertained by proper experiments. Neither has any thing satisfactory been presented in support of this opinion, that an absorption is performed by the absorbent vessels having their mouths upon the skin.

This is a mere conjecture, and from the time of Hypocrates to the present day, it has been taken for granted, that fluids of different kinds are received into our bodies at these openings upon the skin, and from thence carried into the general circulation.

This assertion has been employed to explain the theory of contagious and epidemical diseases; the prolongation of the life of animals deprived of food; the presence in the body of fluids which had not been taken in by the mouth; the identity of effects, produced by several substances, either taken into the alimentary canal, or applied upon the skin; and many other phœnomena, which are well known, and too long to relate.

After consulting several authors who have written upon this subject, I found that they have relied too much upon one another. Some of them have done but little else than to repeat what had been said before. Some others have, it is true, published the result of their experiments, but without any detail of the manner in which those experiments have been performed. Thus, a shadow of doubt is always left in the mind of the reader, and the exactness of the experiments questioned. It is impossible to trace the origin of errors, and to discover whether, or how the experimenter has been deceived : his labour is a mere loss to posterity, and it can never be compared with that of others. Hence a variety of results from the same experiments; errors are perpetuated, and confidence destroyed.

It is the fate of the human mind to be flattered with what favours its own opinion. Men are apt to believe assertions which coincide with their own principles, and are too prompt to establish as axioms, such facts as should only be submitted for investigation.

Besides, so numerous and various are the causes by which nature performs her operations, that we should constantly be on our guard against error. These causes are in a constant activity, but they cannot act upon bodies which are not in that sphere of activity. Those concomitant causes, that submit bodies to that sphere of activity, are only pre-disposing causes, and are inert by themselves. The true or efficient cause, is the one to be attended to, but it is not often easily found out.

If the inquiry into the nature of causes meets with so many difficulties, the ascertaining of their effects is

often no less perplexing ; how many changes happen in the body, before a remedy has produced its effect, which is only to be ascertained when it has ceased to act ; How often is this effect protracted to such a distant period, as to fall out of the observation of the Physicians !

Hence, such a diversity of opinions, often entirely opposed to one another, upon the same subject. Hence, the uncertainty of the action of remedies upon the human body.

The human body being not a simple machine, but an assemblage of a number of systems, very different in their functions, the different connection of those systems with one another, must certainly favour certain phenomena to take place in some individuals, while others cannot be affected in the same manner. But as the principal functions of the animal economy, are performed in such a manner as to have particular organs to perform such functions, it is more than probable, that the function of absorption, if necessary for the support of the animal oeconomy, has been intrusted to a particular organ, fit for elaborating the absorbed matter, and conveying it to its proper destination. This, which I believe to be provided for in the animal oeconomy, I shall endeavour to prove by facts supported by experiments.

From the constant stream of perspirable matter, exhaled from the body in a state of health, as well as from the aqueous fluid running over the surface of the body, when overheated or debilitated by fever, it has been inferred, that there existed a set of vessels, to pour out such exhalation. This supposition being granted, the necessity of explaining several phenomena supposed to be



produced by absorption, has given rise to another supposition, which received sanction by the discovery of a set of vessels existing in the animal body; to wit, the Lymphaticks, whose use is not yet well understood.

This set of vessels, whose origin cannot be traced, but seems to take its rise from the extremities, has received the name of the Absorbent system, and the function of absorption has been intrusted to it.

This opinion has been so generally adopted, that but few experiments have been made, either to support, or to condemn it; theories have been established upon conjectures; every inexplicable phenomenon in the animal oeconomy charged to absorption, and the skin, the only covering given to animals by nature, to defend their bodies from the noxious vapours floating in the fluid in which they are immersed, has been made the door to the receptaculum of such noxious vapours.

I shall not spend time in compiling the results obtained and related by different authors, from their experiments; some contradict one another\*, others have not been accurate, or have attributed to absorption, what was to be attributed to another cause.

In this uncertainty of judging between two authors, who do not agree upon the result of an experiment, I

\* Dr. Bedoes, in his pamphlet (*Considerations on the Medical use of factitious air*), endeavours to prove that oxygenous gas, when breathed into the lungs, or applied to the skin deprived of its cuticle, is productive of inflammation.

Doctor R. T. Ferro, of Vienna, asserts, from his experiments, that the contrary happens, and that the same gas diminishes and removes the disposition to inflammation in the lungs, dissolves extravasated Lymph, diminishes the hectic fever, &c. &c.



think, for my part, when the error cannot be pointed out in the mode of operating, that there is no other way to find out the truth, than to repeat faithfully the same experiments, and vary them in different ways.

In stating the doctrine concerning absorption, it has been admitted, that some substances are absorbed and some others rejected,

Of the different gases, the carbonic acid has been said to be the most readily absorbed by the skin. But this is an error upon the true cause, as I have hinted above. The absorption of this gas sometimes takes place it is true, but not by the skin, and only by the moisture exhaled from the skin; a few experiments upon this gas, will convince any one, how greedily it is absorbed by water.

Had I been prejudiced in favour of the absorption of the carbonic acid gas by the skin, I might have fallen into the same error, but a farther prosecution of the inquiry led me to the true cause.

The application of wet cloaths to the skin, if it has ever been productive of such an effect as preventing thirst, and supporting the animal deprived of nourishment, seems to have produced that effect in no other way than by occasioning a constriction of the pores of the skin, and thereby diminishing perspiration, which generally goes on increasing, in proportion as the strength of the animal decreases.

The presence of the smell of garlic, in the breath of persons who have had this vegetable applied upon some part of their body; the smell of violets imparted to the urine of those who have been exposed to the emanations of turpentine, prove nothing in favour of the absorption

by the lymphatics, as I shall hereafter evidently shew, by a series of experiments which I have made and repeated with all possible accuracy.

After many attempts, I have never been able to introduce any substance into the body, by simple application upon the skin, without previously destroying the cuticle: when I have used frictions, I experienced a diminution of the substance employed, but have never been able to say positively that it had been absorbed.\*

I am aware that the effect of the mercurial frictions will be urged as an argument in favour of the notion of absorption by the skin: but, although I am not able to determine, with certainty, how those frictions produce their effect in the body, I may nevertheless give some plausible hints upon this subject, and I think I may with justice remark here, that no experiment has ever decidedly proved that the mercury is actually received and carried into the system by the absorbents. The mercury must certainly suffer a change before it enters the general circulation; for, this mineral, by trituration with fat, is but more minutely divided and held up between the particles of the fat, which add no more to it than a soft adhesive substance, which holds it in contact with the parts, and prevents it from running, during the friction.

\* It may happen sometimes, that the long and repeated use of frictions, alters sufficiently the cuticle, although it appears sound to our eyes, to permit the introduction of the substance employed in friction. This cannot be denied, and requires a greater number of experiments, than I have done, to prove the contrary. But this will not, I hope, be called absorption, and it is sufficiently probable, that if any fluid is introduced in the course of the circulation, without materially destroying that circulation, it will be carried along with the circulating fluid.



Was the mercury absorbed in frictions, it would, certainly, after a long use be found in some part of the body.

Some authors, I know, have said they have found globules of mercury in the milk of women who had been under its use for some time, but I think this is not sufficiently proved, and from a number of experiments which I have made, I am the more inclined to doubt the fact. I have never been able to discover any marks of the presence of mercury in the urine, milk or other fluids of animals, upon which it had not been spared.

From several experiments, which are not yet sufficient to enable me to say any thing positive upon this subject, I am induced to believe, and it appears probable, that mercury being a substance volatile enough to be capable of rising and diffusing emanations by a moderate degree of heat, as it is proved by Mr. Achard's experience,\* it

\* This gentleman having left a dish containing twenty pounds of mercury over a furnace, which was daily heated, experienced a salivation at the end of several days; as did two other persons who had not quitted the room. This heat he estimates to be at about eighteen degrees of the graduation of Reaumur's thermometer ( $72^{\circ}$  of Farenheit)—*Journal de Physique*, Octobre 1782.

Dr. Barton, Professor of this University, assured me that he was salivated, by only going several times in a close room, where one of his patients was under a profuse salivation, produced by the mercurial frictions.

Dr. Shippen, and Dr. Wistar, both professors of the same University, have also experienced a salivation during their attendance at the Pennsylvania hospital, by their visiting the room where the mercurial frictions were administered to several patients.

Numerous facts of the same kind might be related, upon good authority.

may, with the assistance of the heat of the body of those using the frictions, be raised in very minute particles, as musk, camphor, the spirit of turpentine, garlick, and others—and absorbed in the same manner as these volatile substances are.—May it not be supposed also, that, during the frictions, some of the mercury is, by the action of the air assisted by the heat of the body, oxidized, and that it afterwards parts by degrees with its oxygen, which carries along with it to the lungs, (which I am to prove, are the organs performing absorption,) some parts of the mercury which is with difficulty separated from it?\*

I could present many other conjectures, to shew that mercury, when applied upon the skin, is not out of the sphere of absorption by the lungs, but having determined to offer nothing but facts supported by experiments, I must apologize for this digression, and return to my subject.

From all the facts and observations that I have been able to collect from others, as well as from my own observations and inquiries, I have never been able to determine the absorption of any substance by the lymphatic system.

\* Oxygenous gas obtained from the mercurial oxides, almost always holds a small quantity of mercury in solution: I have been a witness to its having produced a speedy salivation on two persons who used it for disorders in the lungs. In consequence of these observations, I filled bottles with gas, exposed them to an intense cold, and the sides became obscured with a stratum of mercurial oxide, in a state of extreme division. I have several times heated the bath, over which I caused this gas to pass, and I obtained at two different times, a yellow precipitate, in the bottle in which I had received the gas. Chaptal's Elts. of Chemistry.



ticks;\* On the contrary, every experiment that I have made, with the view to come at such a discovery, has convinced me that no such function appears in the animal economy; and that the lungs are the only organs in which the absorption is performed.

Had nature created such a function as absorption by the skin, how should the greater number of animals have their skins covered with furs, feathers, scales or shells? Would not such a covering impede the access of the matter or principle destined to be absorbed?

Why, if the absorbents perform such function as inhaling extraneous substances, do we not find in the course of these vessels, congestions and accumulations of several substances, as we find often in the lungs.\*

## C

\* The most deleterious fluids may be handled with impunity, when care is taken to avoid to breathe the noxious vapour.

Anatomists dissect putrid and disordered bodies with perfect safety, and nothing of the noxious matter is absorbed by their hands as long as the cuticle is not injured.

Would not surgeons be, every day, exposed to the most dangerous consequences, if the matter of venereal and gangrenous ulcers, could be absorbed by the entire skin.

I have myself, some time ago, examined a dead body, so much disordered, that all the instruments that I used in dissecting, were turned black, my hands were no less than four hours soaked in that infectious matter, but I received no injury from it.

I have many times delivered women labouring under the venereal disease, with perfect impunity.

The venom of the viper may be laid upon the skin, without any accident ensuing.

\* What is commonly called *catching cold*, has often been, and is yet daily attributed, by many, to an absorption of moisture; but is it not more plausible to admit that it is the result

But farther, considering the constant atmosphere of perspirable matter, surrounding living bodies, and produced by their own emanations, will it not be difficult to conceive how such a function as absorption by the skin, could ever be beneficial to the animal? How could the matter exhaled, and that inhaled, be prevented from being mixed with one another? There could be no difficulty to crowd objections upon objections, but a refutation is not my object at present.

It is by no means, however, my intention to deny that absorption is performed under the skin, as well as in the different cavities of the body: Neither would I pretend to contradict what is daily proved by experience, upon the virus of the small-pox, and other poisons introduced under the cuticle;\* my endeavours tend only to shew, that the absorption by the skin is not essential, but merely ac-

of the suppression of the usual discharge of the exhaling vessels upon the skin? It is evidently known, that the operation of cold upon the body, is to produce a constriction of the extreme vessels, consequently, the small extremities of the absorbents, would suffer the same constriction, and thus be prevented to receive any such moisture.

The quantity of perspiration is often, it is true, materially affected by the changes of temperature, and yet no injury is the result, because this matter has been determined to the kidneys, and the secretion of urine proportionally augmented. But it certainly happens sometimes, that this perspirable matter is prevented to take its course to the kidneys, and in such a case becomes a productive source of general or local disease, according to the parts which are affected.

\* The introduction of a virus in the skin, or under the cuticle, cannot certainly be called absorption. In such a case the virus is carried on by the circulating fluid, with which it has been mixed.

cidental to the animal economy, and that far from being one of its functions, it cannot take place as long as the cuticle remains sound and entire.

As I am far from desiring to establish a new system, I shall guard against forming any assertion which cannot be supported by facts deduced from experiments.

I shall not confine myself to a simple narrative of facts. Being myself more inclined to doubt than to believe, I wish to give to my readers a sufficient ground to establish an opinion, and a fair chance to point out my errors, in case I have been misled in my enquiries.

I have collected a number of experiments which I had made at different times and in different places; and not to let any doubt exist in my mind, of their exactness, I have repeated and varied them, and compared their results.

To remove all doubts that might arise concerning their accuracy, I thought proper to detail them minutely, and give an account, not only of my proceedings, but of all the concurring or intervening incidents, which might have been the cause of any error.

Therefore, laying aside mere reasoning by which we are so often misled, and having no wish to prejudice my reader in favour of the following experiments, I shall premise nothing further but proceed to recite them in their proper order.



FIRST SERIES  
OF  
EXPERIMENTS.

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EXPERIMENT I.

To satisfy myself of the effect of turpentine upon me, before I would proceed to any experiments; at ten o'clock in the morning, after having breakfasted at eight, I took a few drops of spirit of turpentine, properly diluted with some water and sugar. Soon after, I drank a tumbler of water and sugar, in which I put a few grains of Nitre, to promote the secretion by the urinary passages.

The effect was, as I expected, the same that happens to every person using turpentine internally; to wit, to give a strong smell of violets to my urine.

This, I believe, to be a characteristick of turpentine, and its preparations, for, I do not know that any other substance produces the same effect in the human body.

EXPERIMENT II.

A few days after my first experiment, when I could not suspect any particle of the turpentine existing in my body, taking my urine for a test, I began by trying one of the experiments so often cited, though few, perhaps, have properly attended to it; to wit, exposing myself to the emanations of the spirit of turpentine.

At eleven o'clock in the morning of a clear day, the temperature of the atmosphere being at  $61^{\circ}$  of the thermometer of Farenheit, having breakfasted at half after eight, upon bread and butter and coffee, my pulse beating 70 strokes in a minute, its common standard at that time, I exposed myself, in a close room, to the emanations of the spirit of turpentine, by only pouring some of this spirit into an open bowl which was placed on a table, and walking here and there in the room, without any alteration of my usual dress:

I remained, so circumstanced, for half an hour in the room, after which time, taking the same test as in my first experiment, the only one indeed, and certainly sure, to prove the introduction of the turpentine in the body, I found the same smell of violets and quite as strong, I can say, as I had in my former experiment, where I had taken internally the spirit of turpentine in substance.

I, then, had no doubt of the turpentine, or its emanations, being introduced into my body and carried to the general circulation; but it remained yet to find and determine, with certainty, how it had been introduced.

Not willing to trust to the general opinion, which attributes this phenomenon to absorption, and farther more, if so, desiring to fix in a precise manner how it was done, I proceeded to a third experiment, as follows.

### EXPERIMENT III.

From a number of observations, I had reason to suspect that absorption was effected by means of the organs of respiration, and perhaps by those organs exclusively, to wit, the lungs.

I, therefore, procured a long tube by means of which, I could, when walking in a room, draw the air necessary to supply respiration, from a distant place, where no emanations, of those to which my body was exposed, could either be previously existing or conveyed during the experiment.

At twelve o'clock in the morning of a serene day, the thermometer standing at  $63^{\circ}$ , five days after my last experiment, to leave no suspicion of any particle of the spirit of turpentine existing in my body, my pulse beating 70 strokes in a minute, its common standard at that time, having made my breakfast, at nine o'clock, upon milk and bread, my long tube disposed in such a manner as to have one of its extremities out of the room where I stood, and the other extremity in my mouth, I stopped my nose and breathing through the tube, during the whole time of the experiment, I uncorked the bottle containing the spirit of turpentine and poured it into a bowl, as related in EXPERIMENT II.

I continued exposed to the action of this spirit, in the same manner as I had in my second experiment, for the space of two hours, provided with an assistant, to follow my directions during the time of the experiment.

As I had begun by taking the diuretick draught, as in my former experiments, I was at liberty to try, after an hour was expired, whether any symptom of the presence of turpentine existed in my body, but none could be discovered.

I waited patiently for an hour longer, in the same situation, and had twice recourse to my usual test, during that time and at the expiration of the second hour, but no sign whatever of the smell of violets was exhibited.



Having changed my cloaths, I left the room of my experiments, and retired to a place entirely free from the fragrant smell of turpentine, to be able to continue my observations through the whole day : but no smell of violets being perceivable in my water for twenty-four hours, and indeed any time after the experiment where I was certain to be free from the emanations of the turpentine, I concluded, agreeable to what I had suspected before, that no turpentine had been absorbed.

This might have been satisfactory for any common observer, but I, far from being satisfied, wished for nothing, but going farther into such an important inquiry. I was able to do it a few days after.

#### EXPERIMENT IV.

By the fire-side, in the morning of a beautiful day of the month of March, I prepared every thing to proceed on my experiment.

My long tube, a phial of spirit of turpentine, a large glass jar, and enough of the diuretick potion to make three draughts, to be taken every hour, during the time that my experiment would last.

I took the extremity of my long tube, disposed as described in the preceding experiment, in my mouth, and began to breathe through it.

The phial of spirit of turpentine being handed to me by my attendant, I took it in my right hand, and introducing my arm into the jar as far as the elbow, directed the jar to be luted round my arm in this situation.

I, then, sat down, and after having taken the first draught of the diuretick potion\*, desired my attendant to

\* Water and sugar with a few grains of nitre.

feel my pulse. The number of pulsations in a minute, counted by a good second watch, at three different times, was from 69 to 72, which I judged to be a little mistake in counting, and took it to be 70, its common standard. I uncorked the phial and poured the spirit of turpentine upon my hand inclosed in the glass jar, as described above. My fingers, thumb and metacarpus, were immersed in the liquor and my arm, naked up to the elbow where it was luted to the jar, exposed to its vapour during the whole time of the experiment.

Having remained half an hour in that situation, I directed my attendant to feel my pulse, which was found 75 strokes in a minute ; but thinking that such an increase might be owing to the uneasy posture I was in, I changed it, and my pulse was, immediately restored to its common standard, 70, and continued so during the whole time of the experiment, as it has been ascertained every half hour.

I, now, took the second draught and continued in my former position until an hour was elapsed, at which time I examined my urine, that had been abundantly secreted. Not the least smell of violets was perceivable in it.

I resumed my position, continued in it for an hour longer, and during the mean time, took the third draught that I had prepared. At the end of the second hour, I repeated the examination of my urine, but no smell of violets was found in it, neither by me, nor by my attendant. I, then, desired my attendant to smell my breath, and several parts of my body, but not the least smell of turpentine was to be discovered. The glass jar being perfectly luted round my arm, did not let a single parti-

cle escape, and thus gave a fair chance to discover the smell of turpentine out of my body, in case any had been absorbed and carried into the general circulation.

I thought proper to carry the experiment farther, to create no kind of doubt in my mind, against the consequence which I was to deduce from it. A third hour was spent under such a trial, during which my fingers began to smart at the middle. The pain increasing and a redness appearing upon the parts which had been kept immersed in the spirit of turpentine during the three hours, I put an end to the experiment, washed my hand and arm, as much as necessary to take away the smell of the turpentine, and left the place where the apparatus was, to prevent the emanations of the turpentine from having any access to my lungs.

I made a new examination of my urine which I discharged abundantly from the three large draughts that I had taken during the experiment, but could perceive no kind of smell of violets in it.

The inflammation upon my fingers, lasted the whole day, but went off in the night. I continued my observations as many times as I made water, and never found the least smell that could induce me to believe that any absorption had taken place†.

This last experiment was, certainly, conclusive, and completely satisfied me : but happening to mention its result to some gentlemen of the faculty, this question

## D

† The perspirable matter which had escaped from my hand during the experiment, and mixed with the spirit of turpentine, was about a pap-spoon-full.



was asked of me, "whether I had tried the effect of the spirit of turpentine upon no other parts of the body but the hand." My answer was, I had not ; and to prevent any such objection, that might arise in consequence of the small surface which had been exposed to the action of the spirit, I proceeded to a farther experiment, as follows.

### EXPERIMENT V.

In the beginning of April, the morning being cloudy and a little cold, I lighted a good fire in the room where I proposed to perform my experiment. The thermometer stood at 66° of the scale of Farenheit. My pulse beat 71 strokes in a minute, its natural standard that day.

At 10 o'clock, I began the experiment, having previously disposed my long tube to draw the air, that I wanted for respiration, from without. Provided with an assistant, I took the diuretich draught composed as usual ; undressed myself, stopped my nose, and applied my mouth to my long tube, as in the former experiment.

I, then, directed my assistant to besmear my body, by means of a sponge, with the spirit of turpentine, which office he repeated several times, during the time that the experiment lasted.

In a few minutes my pulse began to rise to 76 strokes in a minute, and continued to rise as I shall say hereafter.

I was then standing up, a little stooped, to keep my tube in my mouth, which position being uneasy, I took the extremity of the tube in one of my hands, and held it to my mouth.

Half an hour being expired, I made water, which being examined and tried by my assistant, discovered no smell of violets. My pulse now was beating 96, and

from that point, which was the highest during the operation, it decreased gradually to 80, which was its standard, when I desisted from any longer trial.

After an hour spent in this laborious and painful experiment, I gave my urine to my assistant, to try whether he could, at that time, find any smell of violets ; but he could not perceive any.

Not being able to support the trial any longer, I ended the experiment, tried my urine myself, and I declare it had not the least sensible smell of violets.

I now could hardly bear the strong smell of turpentine rising from the surface of my body ; perceiving my pulse raised, I applied my fingers to the wrist and counted 106 pulsations in a minute, by my dead-seconds watch, standing up\*.

After I had washed myself, I felt a little of the head-ache, and standing before the fire, with my single shirt on, to look at my watch, which was on the mantle-piece, I found my pulse beat 116 strokes in a minute.

At half after one, sitting at my desk, I again felt my pulse ; it had fallen to 72 strokes in a minute. Rising from my chair and standing up, I found it to be 80 in a minute. The head-ache was not entirely gone, but was very weak. Some redness remained upon the skin of my thighs, but I felt no pain.

I shall make no comment upon this disagreeable and painful experiment, which I think a conclusive one. Few will be tempted to repeat it, but if any should feel an inclination to do it, I must here warn them, to pay the

\* I attribute the increase of my pulse to the effect of the emanations of the spirit of turpentine getting suddenly into my lungs.



utmost attention to their operations ; for, if the nose is not stopped and the tube well adapted to the lips, the air of the room, will, in the inspiration, make its way to the lungs and carry along with it some of the emanations of the spirit of turpentine, which are of the most penetrating kind ; then the experiment is broken and to be repeated at a period sufficiently remote, to leave no suspicion of the existence of the turpentine in the body of the experimenter.

It being, I hope, sufficiently proved by this series of experiments, that the absorption of the turpentine did not take place by the skin, any one might think himself sufficiently authorized to conclude, that in the experiment II, or in any other instance, the absorption did or does take place by the organs of respiration, and no otherwise.

But to be free from all objections upon a subject of so much importance, I thought best to establish this fact beyond all doubts, by another series of experiments, in which I could plainly shew that the phenomenon of absorption which as I have proved, does not take place by the skin, is always performed by the lungs, even when no other parts of the body, but the lungs, are in contact with it.



SECOND SERIES  
OF  
EXPERIMENTS.

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EXPERIMENT I.

About twelve o'clock in the morning of a windy day, I took a narrow-neck'd bottle, containing spirit of turpentine, and applying my nose close to the opening, inspired a dozen of times in that situation, put the bottle by and left the room.

I went about my business, took a walk in the street; and an hour and an half afterwards, the smell of the turpentine was sensible in my breath, to those whom I called upon for trial.

On my first making water after the experiment,\* a strong smell of violets rising from it struck my nose; the urine retained this smell of violets, for twenty-four hours after, and had it yet when it was thrown away.

My water gave the same test, until I went to bed at 11 o'clock, every time I was prompted to ease nature on that point. It deposited a red sediment, and seemed to be secreted in a larger proportion than usual: but these two last symptoms, I am not confident, though I strongly suspect, to be the effect of the turpentine inhaled.

\* One hour after.

I must here remark, that I have several times, during my attendance at the lectures upon chemistry in this university, found my water impregnated with the smell of violets, whenever there was some turpentine inflamed in performing the experiments; which smell could be owing to nothing else, but the smoke of the turpentine inhaled into the lungs. This shews, and it will be again proved hereafter, that, in whatever manner or form this substance is introduced in the body, its effect is constantly the same.

## EXPERIMENT II.

Desirous to try how little of the turpentine was necessary to produce a sensible effect in the system, and how long the absorption was to take place, I emptied my lungs by a long expiration, then inhaled once the emanation of the spirit of turpentine and put the bottle by.

In the space of half an hour, I found my water very sensibly impregnated with the smell of violets, and this effect was yet very remarkable after three or four hours' time.

This experiment shews, how very cautious the experimenter ought to be, in performing the experiments in which the organs of respiration are not to have any share: because, if the smell of the turpentine has once struck the nose, it is enough to render the result of the experiment fallacious.

These two experiments might suffice, to prove that absorption is taken in by the lungs, but to leave no room to those inclined to make objections, I shall relate another experiment, which I think is perfectly decisive.



## EXPERIMENT III.

By means of my long tube, I inhaled ten successive times, the emanations of the spirit of turpentine enclosed in a glass jar and set so far distant from me, that no emanations could reach to the place where I stood. A quarter of an hour after, I tried my urine, which began to emit some smell of violets; I waited a quarter of an hour longer, and upon a second trial, I found it with a smell so considerably increased, that it might be discovered in every part of the room where I was. Half an hour more being expired, the smell was still stronger, but I did not observe any farther increase, and this effect lasted quite as long as it had in my former experiments.

This would have been my last experiment, but I did not feel satisfied unless I could positively ascertain, whether the effect of the turpentine would be the same as generally observed, if introduced into the body, and its emanations prevented being received into the lungs. For I suspected that the turpentine, in its course through the lacteals, before it could be mixed with the blood, might suffer a change to which it cannot be subject, when its emanations are readily brought into contact with the blood in the organs of respiration: this was to be determined by experiment.

## EXPERIMENT IV.

My object was, now, to introduce the spirit of turpentine in the body, without permitting the lungs to receive, at the time of, or after the introduction, the least particle of its emanations. I could find no better way of doing it, than by injection into the rectum, with this precaution



of drawing at the same time my breath from a distant place by means of my long tube, to avoid breathing the smell of the injection when presented to me: This mode of operating, had also the double advantage to satisfy me upon the absorption by the lacteal vessels, and to confirm evidently the possibility of conveying both nourishment and remedies to the system, in many cases where there is either impossibility, or reluctance, to introduce them into the stomach.

I was completely gratified in my expectations, as it will be shewn by the result of the present experiment.

At eleven o'clock in the morning, three hours after my breakfast, my body being in a perfect state of health, my pulse beating 72 strokes in a minute, I directed an assistant to prepare an injection composed with four or five drops of spirit of turpentine, diluted and suspended in about half a pint of luke-warm water by the medium of the yolk of an egg; In the mean time I prepared in another room, (to avoid entirely the emanations of the spirit,) my long tube, and began to breath through it, one end being in the open air out of the house. I introduced almost the whole of that injection into my rectum, and leaving immediately the room, I did not draw my breath until I had got in the open air out of the house.

I shall now give the result of my observations during the operation of the injection.

It neither gave pain nor uneasiness, was kept very easily, and the whole of it absorbed, as I judged by none of it being returned.

In half an hour, hardly any smell of violets was imparted to my urine, but the smell of turpentine began to

be perceptible in my breath, as I was informed by my assistant.

In three quarters of an hour, the smell of violets was evident in the water I made.

In one hour, it was strongly impregnated, and the smell of turpentine increased in my breath.

In one hour and a quarter, it was much increased.

In one hour and an half, it was still stronger.

In two hours, still a little, but very little stronger.

It did not increase any longer, but the impregnation of the system was such, as to impart the smell of violets to my urine the next day.

My urine deposited the same red sediment,\* which I have mentioned in the first experiment of the second series. I discovered no increase in its secretion, until late in the evening. The discharge was also very abundant the next morning, but I am not able to say positively that it was owing to the effect of the turpentine, having not sufficiently paid attention to the quantity of liquid that I drank during, and after the experiment.

The smell of turpentine remained for a considerable time in the lungs, and it was yet very perceivable in my breath the next morning, as I was informed by a lady whom I spoke to.

I felt a heaviness in my head, but no head-ache.†

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\* This effect was protracted during the whole of the next day.

† This last experiment confirms in a striking manner, what has been said by several learned writers upon the function of the lacteals. It shows evidently, that the function of these vessels is not exclusively confined to the lacteals originating from the small intestines, that are spread upon the mesentery, but that it is continued to the very last part of the intestinal canal.



I could adduce a number of other experiments made with musk, garlick, and camphor, giving all the same result, to reinforce all that I have said upon the absorption of the spirit of turpentine, but the small limits of a dissertation do not admit of such an extent as would be necessary to go through such a task.

I have preferred the spirit of turpentine for the subject of my experiments in the present dissertation, and I would recommend it to those who will faithfully repeat the same experiments, on account of its certain characteristick property of imparting a strong smell of violets to urine, whenever and howsoever it is received in the body. Such a smell cannot leave any doubt of its presence in the system, as soon as it has been received; when on the other hand, the proofs given by the other volatile substances of their existence in the body, are very fallacious, and may easily fall out of observations.

From this present course of experiments, as well as from many others that I have made, and which are no

The greater length of time elapsed between the taking in of the turpentine, and the production of its effect upon the urine, the smell imparted to the breath, before any change had appeared in the urine, all these satisfactorily trace the course of the absorbed substance through the lacteals and thoracick duct to the left subclavian vein, into which it is mixed with the blood and carried along with it in its return to the heart by the superior cava: yet unaltered it is propelled by the heart through the lungs, where it undergoes the same oxigenation as the blood, then returned to the posterior auricle which pours it in the same ventricle of the heart, from which it is sent by its muscular contraction through the arteries to all the parts of the body. It at last gives a proof of its return in the kidneys, by the smell of violets imparted to the urine secreted by these glands.



less conclusive, it naturally and evidently flows as true consequences ;

1. That absorption is performed by the lungs in the act of respiration.

2. That it is performed by these organs alone.

3. That no substance can be introduced in the body by mere application upon the skin, when the cuticle is sound and entire.

4. That in the external application of volatile and acrid substances upon the skin, although nothing is introduced in the body, they produce, however, some effect, and this probably by the action of their stimulant power upon the extremities of the nerves spread upon the skin under the epidermis.

I could expatiate upon the present subject, but I am too confident of the abilities of those gentlemen to whom this essay is submitted, not to leave to their own reflections and judgment, the inductions that may be drawn with propriety and perspicuity, from experiments faithfully and accurately performed.

To say that these are sufficient to establish a truth, would certainly be too presumptive, and I am too sensible of the difficulties that are crowded in the way of an experimenter, as well as of the multitude of intervening causes that escape his vigilant observations, to pretend that my experiments will satisfy every body. But as my inquiries are not to terminate here, I hope to be able to establish, at a future period, facts that I only consider here as submitted for investigation.

I cannot conclude without addressing this request as a caution, to those of my readers who may be induced to

repeat my experiments, as well as to those who may be tempted to pursue a farther inquiry, not to do it with the view of entering into a controversy, for, a prejudiced mind is most commonly misled by its passion.

Had I been misled myself in my inquiries, as the safety of the life of a physician and of many others intrusted to his care, depends upon the exactness of his observations, which constitute that, for ever objected to young physicians, grand desideratum called EXPERIENCE, I do sincerely declare that I shall receive as much pleasure from a well-founded refutation, as I will be gratified in having prepared the way to new, important, and useful discoveries.









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